THE BRITISH ASSOCIATION

Reports

Report of the Committee on Experiments to determine the Thermal Conductivities of certain Rocks, by Prof. Herschel and Mr. Lebour.

In the introductory notes on these experiments, published as an appendix (p. 233) in the last volume (for 1873) of the British Association Reports, the list of rocks selected and the manner of experimenting on them were described. With the exception that sections of Calton trap rock, of a great pyramid casing stone (nummulitic limestone), Caen stone (or Normandy building limestone), Cannel coal, chalk, and red brick were added to this list, and that the apparatus received some small but very important improvements to make it heat-tight, the material of the experiments as well as the method of working them remained substantially the same as last year. Instead of a conical tin vessel with 1 lb. of water, a cylindrical one holding $2\frac{1}{2}$ lbs., with an internal agitator and thermometer, was used as the cooler. The opposite faces of the heater and cooler were lined with velvet and each clasped by a caoutchouc collar, which, projecting a little above them, clips the circular edge of the rock plate when it is placed between them, and two small notches cut in each collar also allow the wires of a thermocouple to be introduced, touching the rock-surfaces while the rock is being heated. With the view of traversing the plate with the thermopile in different directions, the piece of stout palladium wire (about 18 gauge) used as the electromotive element between the two iron wire branches of a delicate reflecting galvanometer, was silver-soldered to the iron wires at its two ends, all the wires being first rolled thin and flat to some distance from the junctions. The scythe or scimitar-blade shape most easily given to the wire in rolling it thin was advantageous in the construction, because, instead of uniting the wires continuously in one straight length and folding the points of junction on opposite sides of the rock (thus confining their range upon it to a single diameter or to one straight line), advantage of the curvature was taken to connect the wires by superposition, instead of by prolongation at their junctions, without overlying each other, into two flat ogee arches, or merry-thought-like blades, between which the rock is held as in a forceps. The straight unrolled parts of the wires are bound very firmly to a square piece of wood, which acts as a handle to guide the points of the forceps to various parts of the rock-faces, while it keeps them securely in their places, and thus allows the small elastic pressure of the wires to clasp the rock gently between the points of the thermo-electric pincette without assistance from the velvet covers. After thus inserting a rock section in the apparatus, protecting the rock and cooler from below with a stout wooden screen, and from loss or gain of heat in other directions by a suitably thick case of woollen stuff and a few bandages of similar materials, the rate of rise of temperature in the cooler, when agitated, was noted by the average number of seconds taken by a delicate thermometer contained in it to rise $\frac{1}{5}°$ F. (one graduation on its stem) as soon as this rate of rise was found to have become sensibly constant. About twenty minutes were usually occupied in the beginning of an experiment with waiting for a steady condition of the thermometer readings, and ten or twelve minutes more were required to ensure it and to obtain the average rate of their increase for the rock specimen under observation. The temperature difference shown by the galvanometer at the same time at first rose rapidly to a high maximum and then descended very gradually to a fixed lower reading. The pincette was traversed to and fro over the rock surfaces while the thermometer was being noted, and exhibited during these motions fluctuations answering to about 1° or 2° F. on either side of an average position; corrected for zero of the scale and reduced by trials for this purpose between every two or three experiments to Fahrenheit degrees, the temperature difference thus formed, divided by the quantity of heat transmitted to the cooler per minute, gave the apparent thermal conductivity of the plate. The results in Peclet's units were scarcely more than one-third of what Peclet and other earlier experimenters had obtained. It was obvious that, instead of marking the temperature difference between the two solid contact surfaces of the rock and velvet which they touched, the points of the thermo-electric forceps showed the temperature of the fluid air-bath in which those two surfaces are immersed. The extreme mobility of this integument, enabling it to penetrate through the velvet to the plates of the heater and the cooler, while it equally insinuates itself between the rock surface and the thermopile that can only enter into actual solid contact with each other (at least theoretically) at three points, controls the temperature of the metallic thermometer far more powerfully than the rock face that it touches, and the real temperature differences between the rock faces are accordingly completely masked. It is very probable that if the velvet covers fitted on the instrument were replaced by soft wash-leather, the source of this error would be very much reduced; and although it is certain that the confronting surfaces of the rock and leather faces will nowhere have actually the same temperature, from the existence of a sensible quantity of resisting air between them, so that, as before, the thermopile will not mark the true rock temperature difference, but a mean between that difference and a similar difference for the leather faces, yet the range of this error will be incomparably smaller than in the experiments already made with velvet covers, whose loose texture precludes the possibility of regarding the comparative results now obtained as positively correct, or more than first approximations, from which the errors arising from surface characters of the rock sections tested have not yet been removed.

To obtain the true rock temperature differences, means were taken to cement the thermopile points to the rock with plaster, a course it would be desirable to adopt with as few samples as possible as standards of correction for the rest, on account of the tediousness of the process and the injury that it necessarily entails to the beautifully worked surfaces of many of the plates. If the correction so found to be required can be restricted by the mode of operating to a range of such small limits as to be applicable generally, without appreciable influence of the surface characters, in making its occasional departures from a mean value very sensible, then the reduction factor found by absolute experiments on a few rocks of characteristically rough and smooth or polished surfaces to obtain the true temperature difference for a given heat-flow from the apparent one shown by the thermocouple placed simply between the rock and leather faces will be admissible within the limits of error of the observations to convert a list of apparent conductivities as just supposed to be obtained from a mere comparative table of relative conducting powers to a table of absolute thermal conductivities, in which the errors of the values given will certainly not be greater than would in all probability have been committed had the direct method of absolute measurement been applied separately to each specimen of the list, instead of only to a few rocks, to furnish data on which calculations of the remainder may be founded. Circular discs of linen, well wetted with plaster of Paris, mixed with a little glue or white of egg, were laid over the surfaces of two or three of the rocks, enclosing under them and against the rock (to which they were also plaster-wetted) the two branches of the thermopile pincette. When these had set quite hard under pressure and were thoroughly dried by a gentle heat, they were placed in the apparatus, and a measurement of the absolute temperature difference and accompanying heat-flow was thus obtained, affording the real conductivity and a means of comparing it with the apparent one found by similar observations of the same rock when no plaster was used, and when the points of the thermopile merely pressed against its surface. Thus the thermo-electric difference obtained with the wire couples merely touching the surfaces of white statuary marble between velvet faces was 16°, while for the same heat-flow when the arms of the thermopile were firmly plastered to the marble plate, the temperature difference observed was only 16°·2 *—being more than $2\frac{1}{2}$ times as large a difference in the former as in the latter case. With whinstone the corresponding temperature differences were 26° and 8°·5.—in the proportion of very nearly 3 : 1. A similar experiment was made with cannel coal, of which the conductivity is much less than that of the last-mentioned rocks, the temperature differences obtained being for the same heat-flow in the plain and plastered plate 53°·4 and 39°·7; in the proportion of only 1·37 : 1—a far smaller reduction than was observed in the two foregoing cases. Care is, however, necessary to introduce wet plaster under as well as over the points of the thermopile in cementing them to the rock, that air may be excluded and the junction may be solid, a precaution which was omitted in this case, as plaster without size was used, which in drying sometimes flakes off from the

* The heat-flow through the plate was actually greater in this latter than in the former case in the proportion of about 5 : 4, showing that the rough plaster-washed linen surface received and delivered heat to the velvet covers much more readily than the smoothly-dressed surface of the stone, and the whole resistance was less in the latter than in the former case, although the rock plate itself had been made thicker. The same diminution of the total resistance occurred also in the experiment with plastered whinstone.

rock surface either entirely or in places. This may render an experiment, as that on cannel coal may not impossibly have been, from this cause entirely valueless; yet this result presents itself, with many others met with in the investigation, as very well worth repetition, with fresh precautions and with new arrangements to guard against the possibility of false conclusions.

Adopting for the present, as probably not very far from the truth, a common reduction factor of $2\frac{2}{3}$ as the proportion in which the recorded temperature differences of the plain rock surfaces between velvet faces exceeded the true temperature differences of the surfaces of the rocks examined, and introducing some very small corrections for the thicknesses of the plates, the thermal capacity of the metal cooler, &c., which are all probably (as well as the allowance for heat-absorption in raising the temperature of the rock plates very slowly during the observations) really negligible in comparison with the uncertainty that attaches (except in one or two well-observed cases of absolutely measured temperature differences of the rock faces) to the great majority of the determinations from unknown peculiarities of surface contact and temperature assimilation where air is not excluded from the junctions, or rendered stagnant in its mode of heat transmission, the following table gives the absolute thermal conductivities (in centimetre-gramme-second, or absolute British Association units) thus provisionally obtained, together with a few similar results found by Peclet, Forbes, and Sir William Thomson in rocks differing little in their description from those included in the present list.

Provisional Determinations of Thermal Conductivities of certain Rocks.—First Experimental Results.

Description of Rock.	Thermal conductivity (gramme-water-degree heat units per second) at 1° difference of the faces, through a centimetre cube.	Earlier Observations of Conductivities of similar Rocks.		
			Description of Rock.	Observers.
Grey Aberdeen granite...	·00600			
Red Cornish serpentine	·00483			
Calton trap rock (first specimen)... ...	·00520	·00266	Calton trap rock	Forbes and Thomson.
Whinstone	·00312	·00169	Sand of experimental rock, Thermometer garden	
Kenton sandstone	·00489	·00689	Cragleith sandstone	
Congleton "second grit" stone	·00462			
Slate	·00392			
Alabaster	·00412			
Sicilian white statuary marble	·00559	·0097	Fine-grained grey marble...	Peclet.
Irish fossil ditto	·00559	·0077	Coarse crystalline white ditto	
Devonshire red ditto	·00525	·0058	Fine-grained calcareous stone.	
Italian vein marble (white, grey veins)	·00512	·0047	,, ,, ,,	
Irish green marble	·00507			
Nummulitic limestone (a piece of Great Pyramid casing-stone)	·00433			
Caen (building) limestone	·00395	·0037 ·0035	Coarse-grained lias building stone	Peclet.
Chalk...	·00384			
Black shale (Newcastle-on-Tyne) ...	·00178			
Cannel coal	·00161			
Plaster of Paris (for castings)	·00163	·00145 ·00122	Ordinary fine plaster (made up) ... ... Finest ditto for casting (made up)	Peclet.

The Report of the Committee for superintending the monthly reports of the progress of Chemistry, by Profs. Roscoe and Williamson, was then read by Prof. Roscoe. The report was very short; the committee does not intend to apply to the British Association for a further grant after the present year.

Report of the Committee on Essential Oils, by W. Chandler Roberts.—The following oils have been examined: Wormwood, Citronella, and Cajeput. The actions of phosphorus pentasulphide, of zinc chloride, and of bromine upon the oils were described. The first-named reagent generally acts by removing the elements of water, with formation of terpenes and cymenes. Zinc chloride generally causes decomposition, giving rise to a mixture of hydrocarbons. Bromine usually forms a bromide, which is then decomposed with evolution of hydrobromic acid and water and formation of a cymene.

Various cymenes have been examined, all of which seemed to be the same. The formation of cymenes from terpenes by the action of sulphuric acid has been verified. Cymenes have also been obtained from oil of turpentine by continued fractionation.

The following numbers express the optical properties of some of these oils:—

	Specific refractive energy.	Specific dispersion.
Absinthol	·4887	·0234
Cajeput	·4916	·0251
Citronella	·5213	·0289
Citronellol	·5176	·0284

The conclusion drawn is, that cymene is the central body in these essential oils, to which the other constituents are closely related; the varying amounts of mechanical energy required for the formation of the different isomerides have not as yet been determined.

Dr. Gladstone said that the optical properties of sixteen cymenes had been examined. Some of these were obtained from substances of low, others from substances of high refractive energy, but in all cases the refraction of the cymene was the same; the refraction of a substance depends, therefore, on the constitution of the substance itself.

Report of the Committee on the Estimation of High Temperatures, by J. Dewar, F.R.S.E.—The committee has not carried on any investigations during the past year.

Gold Assays, by W. Chandler Roberts.—Little has been done by the committee during the past year, but they hope to be able to report fully at the next meeting.

Report of the Committee for assisting in the Exploration of the Victoria Cave, Settle, by R. H. Tiddeman, secretary to the committee.

The explorations have been continued throughout the chief part of the year. The Settle Committee have raised by private subscriptions and spent, besides the British Association grant of 50*l.*, a sum of 113*l.* 4*s.* 3*d.* The late determination of a bone which had been found by the committee in the cave in May 1872 as human, by so great an authority as Prof. Busk, induced the committee to pay their chief attention to the question of its position and the relation of the beds in which it occurred to the physical changes to which the district has been from time to time subjected. In order to do this it was necessary to remove a large portion of the tip of the older workings, which had unfortunately accumulated below the entrance of the cave. Beneath this the Romano-Celtic layer was reached, and several objects of bronze, including bracelets, a vinaigrette, and other articles, were obtained. The Romano-Celtic layer was from 1 ft. to 1 ft. 6 in. thick; beneath this was a thickness of 19 ft. of screes, consisting of angular fragments of limestone, which had fallen from the face of the cliff above. This contained no bones whatever, nor the smallest fragment of any rock but the white limestone of which the cliff above is made. But at the base of this a great many boulders were discovered of all dimensions up to 7 ft. in diameter. The number of these boulders and the peculiar conditions of their position render it quite impossible that they can have been brought through any fissure in the roof of the cave, and so washed in later times over the beds containing the human fibula and the remains of the older mammals. The great weight of some of them quite militates against this idea. Another suggestion, that they may not have been left in their present position at the melting of the ice-sheet, but may have fallen from the cliff in comparatively recent times, is also negatived by the complete absence of any evidence of any such fall through the long period represented by the 19 ft. of screes, their occurrence only at the base of the screes, and by the absence of any drift from the cliff above for some distance round. But another strong argument against this supposition lies in the fact that the boulders are so close beneath the cliff, that if all the limestone weathered from the cliff above and now resting on the boulders were restored to the cliff it would project so much further forward that

it would be impossible for them to fall into their present position; yet we know from their position that the boulders were dropped there before any portion of the screes had accumulated, and therefore at a time when the roof of the cave undoubtedly reached much further forward.

The inevitable conclusion is that man lived in Yorkshire with *Elephas antiquus*, *Rhinoceros tichorinus*, *Ursus priscus* and *spelæus*, *Hyæna*, *Bison*, and red deer long before the existence of the great ice-sheet in Northern Britain and Ireland.

Report of the Boulder Committee.

The Rev. W. H. Crosskey read the report of this committee appointed for the purpose of recording the position, height above the sea, lithological characters, size, and origin of the more important erratic blocks, and groups of erratic blocks, of England and Wales, and reporting other matters of interest connected with them. A schedule has been issued by the committee containing detailed questions of the information required. The object of the committee is not speculative, but to collect the facts, with the intention of afterwards proceeding to their classification, and pointing out their relations to the various theories under designation in glacial geology. Districts in which boulders are rarest are of special importance. The evidence regarding the southward extension of the ice sheet and the reach of the waters of the glacial sea depends largely upon their presence and absence; while their method of distribution is full of geological meaning. The necessity for the work of the committee is increased by the fact that all over England and Wales the destruction of boulders is rapidly proceeding. In the midland districts, a map is being constructed in which the approximate number of boulders and the character of the rocks of which they are formed, together with the effect of the configuration of the country on their distribution, will be shown. From the general position of the boulders it is evident that boulders were deposited at several ages. There are (1) boulders of the earliest ice periods, (2) boulders of the period of submergence, distributed in the lower parts of the glacial clays, (3) boulders of the period of the re-elevation of the land. These varieties have yet to be traced to their various sources, and upon this work members of the committee are engaged. It is as impossible to assign all the boulders to one epoch of distribution, as it is to relegate all glacial sands, clays, and gravels to one period. The report contains details regarding boulders of various districts. From Leicestershire, one fact of especial importance is recorded. Below the drift clay and quite distinct from the surface boulders freely scattered over the country, a group of boulders has been exposed in an excavation made in the centre of Leicester, 25 ft. deep, composed of rocks of foreign origin, and suggesting a stranded iceberg of an early period. In the same county, isolated boulders of large size, and groups traceable to sources some miles distant, prove Charnwood Forest to have been a centre of ice-action of considerable intensity. In Warwickshire a great change occurs. The drift-beds are reduced almost to pebbles; and local geologists give the name of boulders to specimens which in other parts would not be regarded as worthy of the name. Striations are faint and rare. Their grouping is remarkable, and they come from all points of the compass. Isolated boulders are recorded from Northumberland, Yorkshire, Lancashire, Devonshire, and Denbighshire. The committee request members of the Association who have received schedules to return them, and desire communications from geologists disposed to assist them in their work.

The Close Time Committee.

A report was read from the Close Time Committee with reference to the desirableness of establishing a close time for the preservation of indigenous animals. After stating the steps which had been taken with reference to this subject in Parliament, the committee stated its belief that the effect of birds' nesting on such kinds of birds as are known to be diminishing is altogether inappreciable, while its effect on those whose numbers are not decreasing may be safely disregarded, and consequently that there is no need of any legislative interference with the practice. The committee believed that the only practicable mode of checking the diminution of such birds as have been proved to be decreasing is the effectual protection of the adults from destruction during the breeding season. While the Sea Birds Preservation Act continued to work successfully, the Wild Birds Protection Act had done little, if anything, towards attaining the objects for which it was passed, and in various quarters gave considerable discontent. Birds commonly known as "wild fowl" were subjected to very great persecution through the inadequacy of the present law to protect them; they were rapidly reducing in number; they were not only innocuous, but were of great value as food. Consequently the committee hoped that the efforts they intended to make on behalf of wild fowl in the next session of Parliament would obtain a general support. Representations as to the inordinate slaughter of seals which took place every spring in the North Atlantic Ocean had been made to some members of the committee. There could be no doubt that such slaughter at that season would soon bring these animals to the verge of extermination, as had been the case in many other parts of the world, and since their destruction would affect a very large trade, their proper protection seemed to be a subject not at all unworthy of the consideration of the Government. The committee requested their reappointment.

SECTIONAL PROCEEDINGS

SECTION A—MATHEMATICS

On the Perturbations of the Compass produced by the rolling of the Ship, by Sir William Thomson.

The heeling error which has been investigated by Airy and Archibald Smith is the deviation of a compass produced by a "steady heel" (as a constant inclination of the ship round a longitudinal axis, approximately horizontal is called). It depends on a horizontal component of the ship's magnetic force, introduced by the inclination; which compounded with the horizontal component existing when the ship is upright, gives the altered horizontal component when the ship is inclined. Regarding only the error of direction and disregarding the change of the intensity of the directing force, we may define the heeling error as the angle between the directions, for the ship upright and for the ship inclined, of the resultant of the horizontal magnetic forces of earth and ship at the position of the compass. These suppositions would be rigorously realised with the compass supported on a point in the ordinary manner if the bearing point were carried by the ship uniformly in a straight line. They are nearly enough realised in a large ship to render inconsiderable the errors due to want of perfect uniformity of the motion of the bearing point if this point is placed anywhere in the "axis of rolling,"* for in a large ship the compass, however placed, is not considerably disturbed by pitching, or by the inequalities of the translatory motion caused by waves.

Hence, supposing the compass placed in the axis of rolling, the perturbation produced in it by the rolling will be solely that due to the variation of the horizontal component of the ship's magnetic force. Such a position of the compass would have one great advantage—that the application of proper magnetic correctors adjusted by trial to do away with the rolling error would perfectly correct the heeling error. To set off against this advantage there are two practical disadvantages: one that the axis of rolling (being always below deck) would not be a convenient position for the ordinary modes of using the compass; the other, far more serious, that in ships, at all events with iron decks, the magnetic disturbance produced by the iron of the ship would probably be so much greater at any point of the axis of rolling than at suitably chosen positions above deck as to more than counterbalance the grand kinetic advantage of the axial position. But careful trials in ships of various classes ought to be made, and it *may* be found that in some cases the compass may with preponderating advantage be placed at the axis of rolling. Hitherto, however, this position for the compass has not been used in ships of any class, and, as we have seen, it is not probable that it can ever be generally adopted for ships of all classes. It is therefore an interesting and important practical problem to determine the perturbations of the compass produced by oscillations or other non-uniform motions of the bearing point.

The general kinetic problem of the compass is to determine the position at any instant of a rigid body, consisting of the needles, framework, and fly card, which for brevity will be called simply the compass, moveable on a bearing point, when this point moves with any given motion. Let the bearing point experience at any instant a given acceleration, α, in any given direction. Let w be the mass (or weight) of the compass, and $g\,w$ the force of gravity upon it, reckoned in kinetic units. The

* One way, probably the best in practice, of finding by observation the position of the axis of rolling is to hang pendulums from points at different levels in the plane through the heel perpendicular to the deck, till one is found which indicates the same degrees of rolling as those found geometrically by observing a graduated scale (or "batten") seen against the horizon.

position of kinetic equilibrium of the compass at that instant is the position in which it would rest under the magnetic forces and a force of *apparent gravity* equal to the resultant of gw and a force αw in the direction opposite to that of α. Now the weight of the compass is so great, and its centre of gravity so low, that the level of the card is scarcely affected sensibly by the greatest magnetic couple experienced by the needles.* Hence, in kinetic equilibrium the plane of the compass card is sensibly perpendicular to the direction of the "apparent gravity" defined above; and the magnetic axis of the needles is the direction of the resultant of the components in this plane of the magnetic forces of earth and ship. Hence it is simply through the *apparent level* at the place in the ship occupied by the compass differing from the true gravitation level that the problem of the kinetic equilibrium position of the compass in a rolling ship differs from the problem of the heeling error referred to above. That we may see the essential peculiarities of our present problem, let there be no magnetic force of the ship herself or cargo. The kinetic equilibrium position of the magnetic axis of the compass will be simply the line of the component of terrestrial magnetic force in the plane of the apparent level. The author then investigates, by a mathematical process, an expression for "the kinetic equilibrium error," which is so named in order to distinguish it from the error actually exhibited by the compass. The kinetic equilibrium error is exactly the error which would be shown by an ideal compass with infinitely short period of vibration. A light quick needle (either with silk fibre suspension or supported on a point in the ordinary way), having a period of not more than about two seconds, shows the rolling error very beautifully, taking at every instant almost exactly the position of kinetic equilibrium. Sir W. Thomson has thus found it so great in a small wooden sailing vessel that it became very difficult to make exact observations with the quick compass, either in the Firth of Clyde or out at sea on the Atlantic, unless when the sea was exceptionally smooth. The kinetic theory of forced oscillations is readily applied to calculate, whether for a wooden or an iron ship, the actual "rolling error" of the compass from the "kinetic equilibrium error," but the author remarks that it would extend the present communication too far to enter on details of this solution. For the present it is enough to say that no admissible degree of viscous resistance can make the rolling error small enough for practical convenience, unless also the period of the compass is longer than that of any considerable rolling to which the ship may be subjected. Probably a period of from fifteen to twenty seconds (such as an ordinary compass has) may be found necessary for general use at sea; and it becomes an important practical question how this is best to be obtained, consistently with the smallness of the compass needles necessary for a thoroughly satisfactory application of the system of magnetic correctors, by which the Astronomer Royal proposed to cause the compass on an iron ship to point correct magnetic courses on all points.

On the Spectrum of Coggia's Comet, by Dr. Huggins.—The new point noticed in this communication was that the bands of the comet were so far shifted as to indicate—supposing there really was carbon in the comet—that the relative motion of the approach of the comet to the earth was forty-six miles per second. The comet really, however, approached the earth at the rate of twenty-four miles per second; and it was therefore uncertain whether the whole or part of the difference in this velocity was due to the motion of matter within the comet. The brighter portion of the head of the comet was due evidently to a larger proportion of the matter giving a continuous spectrum. It seemed probable, therefore, to the author that the nucleus was solid, heated by the sun and throwing out matter which formed the coma and tail; and part of this was in a gaseous form, giving the spectra of bright lines. The other portion existed probably in small incandescent particles; the polariscope showing that certainly not more than one-fifth of the whole light was reflected solar light.

Further Experiments on Light with circularly ruled plates of glass, by Philip Braham, F.C.S.

Interposing plates of circularly ruled glass in the beam of light from a heliostat, the rings of colour are in the same order by reflection and refraction, the red in both cases being outward.

Observing the rings of reflected colour when the unruled surface of the glass is away from the heliostat, dark bands make their appearance concentric with the coloured rings, if the surface of the rulings is at right angles to the direction of the beam, and on altering the angle of the ruled plate the dark bands expand until they intersect the coloured circles, and also appear considerably beyond them.

Placing a polished plate of speculum metal in contact with the ruled surface of the glass increases the intensity of the dark bands, and by adjustment shows that according to the distance of the reflecting surface from the ruled, the number and thickness of the dark bands are increased or diminished.

A description was given of the heliostat used, the reflector being a rectangular glass prism.

* Generally no adjusting counterpoise for the compass is required when a ship goes from extreme north to extreme south magnetic latitudes.

SECTION B—CHEMICAL SCIENCE

The Chemical Composition of Jute Fibre, by Prof. Hodges.—The jute plant belongs to the family Tiliaceæ. The *Corchorus capsularis* and *C. olitorius* are both cultivated.

The structure of the fibre is different from that of other textile fibres, the central space being very irregular, varying from the thickness of a line to a considerable width.

By the action of aniline sulphate, jute fibre becomes of a golden yellow colour, whereby it is distinguished from hemp and flax. The following is the analysis of jute fibre:—

Wax and fatty matter soluble in ether	0·235
Tannic acid and colouring matter soluble in alcohol	1·135
Sugar, pectine, &c.	2·427
Soluble nitrogenised matters	0·512
Insoluble ,, ,, ...	2·433
Inorganic matter combined with fibre	1·010
Cellular fibre	92·248
	100·000
Nitrogen in original fibre	0·291
Nitrogen in fibre after treatment with solvents ...	0·210

Methyl-thetine, by Prof. Crum Brown and Dr. E. A. Letts.—By the action of bromacetic acid on methyl-sulphide, methyl-thetine hydrobromate is produced. By the action of moist silver oxide on this hydrobromate, silver bromide is found, and by the further cautious addition of the hydrobromate, the silver remaining in solution is removed. By evaporation, crystals of the base methyl-thetine are formed with one molecule of water. This crystallised base might be represented by the structural formula:—

$$\left.\begin{array}{c} S(CH_3)_2 \\ | \\ H-C-COO \\ | \\ H \end{array}\right\} + H_2O.$$

By the decomposition of the sulphate of methyl-thetine by means of barium carbonate, the base may also be prepared.

This substance, methyl-thetine, has both a basic and an acid character; with hydrochloric acid it forms a hydrochloride, from which the double platinum chloride has been prepared. A double lead compound containing $2PbBr_2$ has also been prepared.

The action of iodacetic acid on methyl-sulphide does not give rise to the formation of methyl-thetine hydriodate, as might have been expected; but various substances are formed, among which is trimethyl-sulphine iodide.

Experiments on High Pressures, by Dr. Andrews, F.R.S.—The author entered into full details of the methods of preparing and using his well-known tubes for the production of high pressures. If a mixture of nitrogen and carbonic acid be subjected to high pressures (to 290 atmospheres), no trace of liquid is produced.

On the Latent Heat of Liquefied Gases, by J. Dewar, F.R.S.E.—The author has deduced a formula for calculating the latent heat of a gas from the known tension of that gas. The results of this investigation have already been communicated to Section A.

On Spontaneous Generation from a Chemical Point of View, by Dr. Debus, F.R.S.—To the question, "Has Nature ever produced organic substances from strictly inorganic materials?" Chemistry (according to the author) answers, "No!"

On the Estimation of Phosphoric Acid in Pyrophosphate of Magnesia, by Mr. Ogilvie.—The author's experiments lead him to conclude that this process cannot be relied upon unless taken in conjunction with some other, such as the Molybdate process. The influence of a great excess of magnesia, of ammonic oxalate,

of citric acid, and of alumina or iron oxide, introduces sources of error.

On an Improved Form of Filter Pump, by W. Jesse Lovett.—This pump, which is very simple and appears to give good results, has already been described in the *Chemical News*.

On Sulphur-Urea, by Prof. Emerson Reynolds.—By heating dry ammonium sulphocyanate, sulphur-urea—as has been before shown by the author—is obtained. If the heat be maintained at 170°, about 26 per cent. of urea is obtained in one hour. By the action of metallic oxides in solution on sulphur-urea, metallic sulphides are obtained, together with cyanamide, which, by the prolonged action of water, is changed into dicyanamide.

On the Joint Action of Carbonic Acid and Cyanogen on Oxide of Iron, and on Metallic Iron, by Lowthian Bell, F.R.S.—The author shows that a mixture of carbonic acid (CO_2) and cyanogen exercises a powerful reducing action at a high temperature upon ferric oxide. With one volume of cyanogen and six volumes of carbonic acid at a temperature of 685° to 710° F., 79·9 per cent. of the oxygen existing in combination with iron was removed, 56·3 per cent. of metallic iron being produced; by increasing the proportion of carbonic acid to fifteen volumes, 6·5 per cent. of metallic iron was produced, while ·9 per cent. only was formed when the carbonic acid amounted to thirty volumes. A certain amount of carbon is simultaneously deposited in the reduced iron. The reducing and carbon depositing power of a mixture of cyanogen and carbonic acid is greater than that of a mixture of carbon monoxide and carbonic acid.

Electrolytic Experiments on Metallic Chlorides, by Dr. Gladstone, F.R.S., and Mr. Tribe.—The authors show that if plates of copper and platinum be immersed in a dilute solution of cupric chloride, a current is obtained from the copper to the platinum, the cupric chloride is broken up into cuprous chloride and chlorine, the former being deposited on the platinum, while the latter combines with some of the copper to form a new cupric chloride $Cu + CuCl_2 = 2\,(CuCl)$. By applying an external current the same action takes place; if, however, the strength of the current be increased, the free chlorine makes its appearance. By substituting zinc for copper a greater effect is obtained; with magnesium in the place of zinc, the effect is still greater. Analogously to the foregoing, a current may be obtained by acting on a solution of mercuric chloride with gold and mercury, whereby mercurous chloride is deposited on the gold.

Composition of certain kinds of Food, by W. J. Cooper.—The author drew attention to the nourishing properties of farinaceous foods, such as arrowroot, corn flour, &c. Such foods he believes very well suited for infants and invalids. He holds that we generally take too much nitrogenous substance in our dietaries.

SECTION C—Geology

Prof. Harkness, at the request of the Committee of the Section, described briefly the geological features of the North of Ireland. The relations of the Silurian rocks to the Lower Silurians of Sutherlandshire and Cumberland were discussed, and the later formations were noticed in succession.

Prof. H. A. Nicholson exhibited and described a silicified chip of wood from the Rocky Mountains. At the Brighton meeting the same specimen was shown, when the opinion was expressed that its wood-like appearance was due to mineral structure. The chip was then regarded by some members of the Section as a hornblendic mineral, known as "rock-wood." Subsequent examination has shown conclusively that the specimen is undoubtedly true silicified wood. The age of the chip and the circumstances of its production present many points of interest. The author considers it a prehistoric relic, produced by an axe, which was probably formed out of the native copper so frequent in various paris of North America.

Prof. Harkness accepted the views of the author, and withdrew his previous opinion that the specimen was merely a hornblendic mineral.

SECTION D—Biology

Department of Zoology and Botany

Mr. Gwyn Jeffreys read a paper *On additions to British Mollusca, and notices of rare species from the deep water off the western coast of Ireland*. As many as forty-seven species of molluscs new to science have been yielded as the results of the dredgings in the *Porcupine*, eighty-four new to the British Isles, and 124 new to Ireland, in addition to a number of other species hitherto considered to exist only in the fossil condition, some of them as low down as the Crag. Dr. Carpenter called attention to the enormous importance of these dredging expeditions, not so much from the number of new species discovered by them as from the light which they seem likely to shed on the question of the continuity of forms of life from one geological age to another. The dredgings off Ushant at a depth of nearly three miles have been especially prolific of results. Dr. Carpenter held out some hopes that the Government might be induced to undertake the expense of a dredging expedition in our own deep seas.

Mr. P. L. Sclater read a paper *On the distribution of the species of Cassowaries*. Until very recently there was supposed to be only one species of *Casuarius*; now at least seven species are known, each with a distinct and very limited area, the genus being entirely confined to Northern Australia, New Guinea, and the adjacent isles. A full exploration of New Guinea would probably lead to the discovery of a large number of most interesting new species.

On the cause of the potato disease and the means of its prevention, by Mr. J. Torbitt. The idea thrown out in the paper was that the disease is owing to the gradual natural decay of particular varieties which never have more than a limited length of life in a thoroughly healthy condition, a view which was combated by most of the gentlemen who took part in the discussion. Mr. Carruthers described the mode in which the spores of the *Peronospora* germinate in enormous numbers on the surface of the potato plant, the germinating filaments, however, only developing to a very limited extent and dying away unless abundantly supplied with moisture. It is only by this means that they are enabled to penetrate into the internal tissues through the stomates. Prof. Du Barry's recent researches seem to point to the possibility that we have in the *Peronospora* an instance of "alternation of generations," one generation only being at present known, the other generation possibly presenting an altogether different appearance, and germinating upon some totally different plant.

Prof. Macalister read *Notes on the specimen of Selache maximus lately caught at Innisboffin*.

Further Researches on Eozoön canadense, by Dr. Carpenter. After an historical account of the controversy respecting this organism, the author proceeded to give additional reasons, the results of recent investigations, for concluding the organic nature of the organism, in opposition to the views entertained by Profs. King and Rowney, of Galway. He took the opportunity of contradicting the assertion made by those gentlemen that Prof. Max Schultze had just before his death stated his conversion to their views. Mr. Gwyn Jeffreys, Prof. Macalister, and Prof. Percival Wright expressed their general concurrence in Dr. Carpenter's views.

SECTION G

Mechanical Science

Opening Address by the President, Prof. James Thompson, LL.D.

For a number of years past it has been customary in this andf other sections of the British Association for the Advancement os Science, that the president should give an introductory addrest at the opening of each new session. In compliance with than usage, I propose now to offer to you a few brief remarks oe various subjects of mechanical science and practice. These subjects have not been chosen on any systematic plan. I have not aimed at bringing under review the whole or any large number of the most important subjects at present worthy of special notice in engineering or in mechanics generally. I intend merely to speak of a few matters which have happened to come under my notice, or have engaged my attention, and which appear to me to be interesting through their novelty or through their important progress in recent times, or to merit attention as subjects in which amendment and future progress are to be desired.

In railway engineering, one of the most important topics for consideration, as it appears to me, is that which relates to the abatement of dangers in the conducting of the traffic. The traffic of many of our old railways has become enormously increased in recent years. With the construction of new lines the numbers of junctions, stations, and sidings have been greatly in

creased; and each of these entails some attendant dangers. As a natural consequence of the increased traffic on old railways, the additional traffic on new lines, and the increased complexity of the railway system as a whole, there have been during recent years more numerous accidents than in the earlier times of railways. It is to be recollected, however, that with a greater number of people travelling daily, more numerous accidents might be expected, and that their increased frequency, on the whole, does not necessarily indicate increased danger to the individual traveller. Referring to the statistics of railway accidents published by the Board of Trade in Capt. Tyler's Report for the year 1873, I find, for various periods during the last twenty-seven years, throughout the United Kingdom, the proportion of passengers killed from all causes beyond their own control, to the number of passengers carried, to have been, in round numbers:—

Proportion of number killed to number carried in the three years 1847, 1848, and 1849,	1 in 4,782,000
In the four years, 1856, 57, 58, and 59 .	1 in 8,708,000
In the four years, 1866, 67, 68, and 69 .	1 in 12,941,000
In the three years, 1870, 71, and 72 . .	1 in 11,124,000
And in the single year 1873	1 in 11,381,000

It is thus gratifying to observe, that in spite of the increased risks naturally tending to arise through the increased and more crowded traffic and the more complicated connections of lines, the danger to the individual traveller is now less than half what it was 26 years ago; at least this result is indicated, in so far as we can judge, from the statistics of deaths of passengers from causes beyond their own control. That the conducting of the traffic of railways still involves hazards far from inconsiderable, and that we have much to wish for towards abatement of dangers of numerous kinds, is proved by the fact that during the single year 1873 there have been killed of the officers and servants of the railway companies in the United Kingdom, 1 out of every 323: so that, at this rate, extended through a period of, for example, 20 years' service, there would be 1 out of every 16 of the officers and servants killed.

These deaths of officers and servants are not to be supposed to be caused in any large proportion by collisions, and by other accidents to trains in rapid motion. The great majority of them arise in shunting and other operations at stations and along the lines, and occur in numerous ways not beyond the control of the individuals themselves. In respect to the passengers, too, it ought to be known and distinctly recollected, that although collisions and other violent accidents to trains in rapid motion, together with other accidents beyond the control of the individuals, usually cause by far the deepest impression on the public mind; yet the numbers of these fatal accidents are small in comparison with others arising to passengers from causes more or less within their own control. For instance, it may be noticed that in last year, the year 1873, while the deaths of passengers arising from all causes beyond their own control, in the United Kingdom, were only 40 in number, there were four times as many killed, namely 160, in other ways; and of these there were as many as 62 killed in the simple way of their falling between carriages and platforms.

In respect to the conducting of the traffic of the trains in motion, it appears to me, on the whole, that when we consider the vast complexity of the operations involved in working many of our ramified and crowded railways, and when we consider the indefinitely numerous things which must individually be in proper order for their duty, and must be properly worked in due harmony by men far away from one another, some stationed on the land, and others rushing along on the engines or trains, the wonder is, not that we should have numerous accidents, but that accidents should not be of far more frequent occurrence. There can be no doubt, however, but that of the accidents which do occur, many arise from causes of kinds more or less preventible according to the greater or less degree in which due precautions may be adopted.

Gradually, during a period of 20 or 30 years past, a very fine system of watching, signalling, and otherwise arranging for the safety of trains, has been contrived and very generally introduced along our principal lines of railway. In saying this, I allude chiefly to the block system of working railways, with the aid of telegraphic signals and interlocking mechanisms for the working of the points and signals.

In former times it was customary to allow a certain number of minutes to elapse after a train passed any station, or junction, or level crossing, or other point where a servant of the company was stationed, before the succeeding train was allowed to pass the same place. Thus, at numerous points along the line a time interval was preserved between successive trains. It was quite possible, however, that the foremost of the two trains, after passing any of these places where signals were given, might become disabled, or might otherwise be made to go slowly, and that the following train might overtake it, and come into violent collision with it from behind. In order to provide against the occurrence of such accidents, a system was introduced called the *Block System;* and its main principle consists in dividing the line into suitable lengths, each of which is called a *block section*, and allowing no engine or train to enter a block section until the previous engine or train has quitted that portion of the line. In this way a space interval of at least the length of a block section is preserved between the two trains at the moment of the later train's passing each place for signalling, and the risk of this space interval becoming dangerously small by negligence or other accidental circumstances, as the later train approaches the next place for signalling, is almost entirely avoided.

Further, at each signalling station, the various levers or handles for working the points, and those for working the semaphore signals for guiding the engine-drivers, instead of being, as was formerly the case, scattered about in various situations adjacent to the signalling station, and worked often, some by one man and some by another, without sufficient mutual understanding and without due harmony of action, are now usually all brought together into one apartment called the signal cabin. This cabin, like a watch-tower, is usually elevated considerably above the ground, and is formed with ample windows or glass sides, so as to afford good views of the railway to the man who works the levers for the semaphores and points, and who transmits, by electricity, signals to the next cabins on both sides of his own, and, when necessary, to other stations along the line of railway.

The interlocking of the mechanisms for working the points and for working the semaphores which, by the signals they show, control the engine-drivers, consists in having the levers by which the pointsman works these points and signals, so connected that the man in charge cannot, or scarcely can, put one into a position that would endanger a train, without his having previously the necessary danger signal or signals standing so as to warn the engine-driver against approaching too near to the place of danger.

The latest important step in the development and application of the block system is one which has just now been made in Scotland, on the Caledonian Railway. Before explaining its principle, I have first to mention that a semaphore arm raised to the horizontal position is the established danger-signal, or signal or debarring an engine-driver from going past the place where the signal is given. Now, the ordinary practice has been, and still is, to keep the semaphore arm down from that level position, and so to leave the line open for trains to pass, except when the line is blocked by a train or other source of danger on the block section in front of that semaphore, and only to raise the semaphore arm exceptionally as a signal of danger in front. The new change, or improvement, now made on the Caledonian Railway consists mainly in arranging that along a line of railway the semaphore arms are to be regularly and ordinarily kept up in the horizontal position for prohibiting the passage of any train, and that each is only to be put down when an approaching train is, by an electric signal from the cabin behind, announced to the man in charge of that semaphore, as having entered on the block section behind, and when, further, that man has, by an electrical signal sent forward to the next cabin in advance, inquired whether the section in advance of his own cabin is clear, and has received in return an electrical signal meaning "*The line is clear: you may put down your debarring signal, and let the train pass your cabin.*" The main effect of this is, that along a line of railway the signals are to be regularly and ordinarily standing up in the debarring position against allowing any train to pass; but that just as each train approaches, and usually before it has come in sight, they go down almost as if by magic, and so open the way in front of the train, if the line is ascertained to be duly safe in front; and that immediately on the passage of the train they go up again, and by remaining up keep the road closed against any engine or train whose approach has not been duly announced in advance so as to be known at the first and second cabins in front of it, and kept closed, unless the entire block section between those two cabins is known to have been left clear by the last preceding

engine or train having quitted it; and is sufficiently presumed not to have met with any other obstruction, by shunting of carriages or waggons, or by accident, or in any other way.

This new arrangement, which appears to be a very important improvement, has already been brought into action with success on several sections of the Caledonian Railway; and it is being extended as rapidly as possible on the lines of the Caledonian Company, where the ordinary mode of working the block system has hitherto been adopted.

The mechanisms and arrangements I have now briefly mentioned are only a portion of the numerous contrivances in use for abatement of danger in railway traffic. It is to be understood that by no mechanisms whatever can perfect immunity from accidents be expected. The mechanisms are liable to break or to go wrong. They must be worked by men, and the men are liable to make mistakes or failures. We shall continue to have accidents; but, if we cannot do away with every danger, that is no reason why we should not abate as many dangers as we can.

Within the past twenty years very remarkable progress has been made in steam navigation generally, and more especially, I would say, in oceanic steam navigation. In this we meet with the realisation of great practical results from the combination of improved mechanical appliances, and of physical processes depending on a more advanced knowledge of thermodynamic science.

The progress in oceanic steam navigation is due mainly to the introduction jointly of the screw propeller, the compound engine, steam jacketing of the cylinders, superheated steam, and the surface-condenser.

The screw propeller, in its original struggle for existence, when it came into competition with its more fully developed rival, the paddle-wheel, met with favouring circumstances in the want then strongly felt of means suitable for giving a small auxiliary steam-power to ships arranged for being chiefly propelled by sails. For the accomplishment of this end the paddle-wheel was ill suited; and so the screw propeller got a good beginning for use on long oceanic voyages. Afterwards, in the course of years, there followed a long series of new inventions and improved designs in the adaptation of the steam-engine for working advantageously with the new propeller; and it has resulted that now, instead of the screw being used as an auxiliary to the sails, the sails are more commonly provided as auxiliaries to the screw. For long oceanic voyages it became very important or essential to get better economy in the consumption of fuel. In order to economise fuel, high-pressure steam, with a high degree of expansion and with condensation, was necessary. This led to the practical adaptation for the propulsion of vessels of the compound engine, an old invention which originated with Hornblower in the latter part of the last century, and was afterwards further developed by Wolff. The high degrees of expansion could not be advantageously used in cylinders heated only by the ordinary supply of steam admitted to them for driving the piston; and more especially when that steam was boiled off directly from water without the introduction of additional heat to it after its evaporation. The knowledge of this, which was derived through important advances made in thermo-dynamic science, led to the introduction into ordinary use in steam navigation of steam-jacketed cylinders, and to the ordinary use also of superheated steam. With increased efforts towards economy of space in the hold of the ship, which became the more essential when very long voyages were to be undertaken, and with the new requirement of greatly increased pressure in the steam, the old marine boilers, with their flues of riveted plates, were superseded by tubular boilers more compact in their dimensions and better adapted for resisting the high pressure of the steam. In connection with these various changes the old difficulty of the growth of stony incrustations in the boilers became aggravated rather than in any way diminished. As the only available remedy for this, there ensued the practical development and the very general introduction of the previously known but scarcely at all used principle of surface condensation instead of condensation by injection. A supply of distilled water from the condenser is thus maintained for feeding the boilers, and incrustations are avoided. The consumption of coal is often found now to be reduced to about 2 lbs. per indicated horse-power per hour, from having been 4 or 5 lbs. in good engines in times previous to about twenty years ago.

Before the times of ocean telegraph cables, very little had been done in deep-sea sounding; but when the laying of ocean cables came first to be contemplated, and when it came afterwards to be realised, the obtaining of numerous soundings became a matter of essential practical importance. In the ordinary practice of deep-sea sounding, as carried on, both before and since the times of ocean telegraph cables, until a year or two ago, a hempen rope or cord was used as the sounding line, and a very heavy sinker, usually weighing from two to four hundred-weight, was required to draw down the hempen line with sufficient speed, because the frictional resistance of the water to that large and rough line moving at any suitable speed was very great. The sinker could not be brought up again from great depths; and arrangements were provided, by means of a kind of trigger apparatus, so that when the bottom was reached the sinker was detached from the line and was left lying lost on the bottom; the line being drawn up without the sinker, but with only a tube, of no great weight, adapted for receiving and carrying away a specimen of the bottom. For the operation of drawing up the hempen line with this tube attached, steam power has been ordinarily used, and practically must be regarded as necessary.

A great improvement has within the last two or three years been devised and practically developed by Sir William Thomson. Instead of using a hempen sounding line, or a cord of any kind, he uses a single steel wire of the kind manufactured as pianoforte wire. He has devised a new machine for letting down into the sea the wire with its sinker, and for bringing both the wire and the sinker up again when the bottom has been reached. With his apparatus, in its earliest arrangement and before it had arrived at its present advanced condition of improvement, he sounded, in June 1872, in the Bay of Biscay, in a depth of 2,700 fathoms, or a little more than three miles, and brought up again his sinker of 30 lbs. weight, after it had touched the bottom; and brought up also an abundant specimen of ooze from the bottom, in a suitably arranged tube attached at the lower end of the sinker.

An important feature in his machine consists in a friction-brake arrangement, by which an exactly adjusted resistance can be applied to the drum or pulley which holds the wire coiled round its circumference, and which, on being allowed to revolve, lets the wire run off it down into the sea. The resistance is adjusted so as to be always less than enough to bear up the weight of the lead or iron sinker, together with the weight of the suspending wire, and more than enough to bear up the weight of the wire alone. Thus it results that the arrival of the sinker at the bottom is indicated very exactly on board the ship by the sudden cessation of the revolving motion of the drum from which the wire was unrolling.

Another novel feature of great importance consists in the introduction of an additional hauling-up drum or pulley arranged to act as an auxiliary to the main drum during the hauling-up process. The auxiliary drum has the wire passed once or twice round its circumference at the time of hauling up, and is turned by men so as to give to the wire extending from it into the sea most of the pull requisite for drawing it up out of the sea, and it passes the wire forward to the main drum, there to be rolled in coils, relieved from the severe pull of the wire and sinker hanging in the water. Thus the main drum is saved from being crushed or crumpled by the excessive inward pressure which would result from two or three thousand coils of very tight wire, if that drum unaided were required to do the whole work of hauling up the wire and sinker.

The wire, though exposed to the sea-water, is preserved against rust by being kept constantly, when out of use, either immersed in or moistened with caustic soda. The fact that steel and iron may be preserved from rust by alkali is well known to chemists, and is considered to result from the effect of the alkali in neutralising the carbonic acid contained in the water, as the carbonic acid appears to be the chief cause of the rusting of steel and iron.

This new method of sounding, depending on the use of pianoforte wire, was first publicly explained by Sir William Thomson in the Mechanical Section of the British Association at the Brighton meeting two years ago; and in the interval which has since elapsed, it has come rapidly into important practical use.

I have to-day already brought under your notice a system of elaborately contrived and extensively practised methods of signalling and otherwise arranging for the safety of trains in motion on railways. These methods, in the aggregate, as we have them at present, may be looked on as the result of a gradual development, which, through design and intelligent

selection, has been taking place during the last twenty or thirty years, or more. In contrast with this I have now to mention a reform towards abatement of dangers at sea, which at present is only in an incipient stage of its practical application, but which I am sure must soon grow into one of the important reforms of the future. I refer to the provision of means whereby every important lighthouse shall, as soon as it is descried, not only make known to the navigator that a light is visible, but also that it shall give him the much more important information of what light it is; that, in fact, it shall distinguish itself to him from all other lights either stationed on land or carried by ships out at sea. The rendering of lighthouses each readily distinguishable from every other light, by rapid timed occultations, was urged on public attention by Charles Babbage about twenty or twenty-three years ago, in connection with a like proposal of his for telegraphic signalling by occulting lights. His admirable idea, however, so far as it related to the distinguishing of lighthouses, has unhappily been left almost entirely neglected until quite recently. Although I say it was almost entirely neglected, yet very important steps in the direction of the object proposed were taken many years ago by Messrs. Stevenson, engineers to the Commissioners of Northern Lights, and the flashing and intermittent lights introduced by them, and now used, although too sparingly, in various parts of the world, constituted a very great improvement in respect to distinctiveness. The first practical introduction of an intermittent extinction of a gaslight, which is a method now likely to become fruitful in important applications with further developments, was made many years ago by Mr. Wilson at Troon; and an admirable application of this plan by the Messrs. Stevenson to carry out the principle of rapid signalling is to be seen in the Ardrossan Harbour light, which is alternately visible for two seconds, and then for two seconds is so nearly extinguished as to be invisible. The whole period—four seconds—is, I suppose, the shortest of any lighthouse in the world. This light fulfils the condition of being known to be the light which it is, within five or ten seconds of its being first perceived; and thus, in respect to distinctiveness, I trust that I may without mistake say it is the best light in the world. Mr. John Wigham has succeeded in constructing large burners for the combustion of gas in lighthouses in general, including those of the first order, and embracing both fixed lights and revolving lights. He has also, in both these cases, applied with the most striking success the principle of occultation. Dr. Tyndall, in his reports to the Board of Trade, has dwelt frequently and emphatically on the ease with which gas lends itself to the individualisation of lights. By its application, he affirms that by simple arrangements it would be possible to make every lighthouse declare its own name. Within about the last two or three years the subject has been taken up energetically by Sir William Thomson. He has become strongly impressed with the enormous importance of the object in question. He has perseveringly laboured in making trials in various ways, both by the method of partially extinguishing gas flames and by the method of revolving screens; and I have pleasure in stating that, as a result of his efforts, a self-signalling apparatus is now constructed for the Belfast Harbour Commissioners, who are preparing to bring it into immediate use at the screw-pile lighthouse at the entrance of the harbour of Belfast. I shall not now enter on any description of this arrangement, as I understand that the apparatus, which has already been temporarily erected for trial in the lighthouse, and has shown good results, is to be exhibited and explained to this Section by Mr. Bottomley, who, as a member of the Board of Harbour Commissioners, has taken an active part in the promotion of the undertaking.

I wish next to make mention of the very remarkable works at present in progress in the harbour of Dublin, under the designs and under the charge of Mr. Bindon Stoney. In order to form quay walls with their foundations necessarily deep under water, he constructs on land gigantic blocks of artificial stone, or, as we may say, of concrete masonry, each of which is about 350 tons in weight, and which are accurately formed to a required shape. After the solidification of the concrete, he carries them away and deposits them on an accurately levelled bottom of sea, so that they fit closely together, and form so much of the quay wall in height as to reach above the low tide level; and so as to allow of the completion of the wall above by building in the usual manner by tidal work, and to allow of the whole structure being carried out without the use of coffer-dams. These operations are on a scale of magnitude far surpassing anything done before in the construction and moving of artificial stone blocks. They are carried out with machinery and other appliances for the removal and the placing of the blocks, and for other requirements of the undertaking, which are remarkable for boldness of conception and ingenuity of contrivance. The new methods of construction devised and applied in these works by Mr. Stoney are recognised as being admirably suited for the local circumstances of the site of the works in the harbour of Dublin, and their various arrangements form a very important extension of the methods of construction available to engineers for river and harbour works.

While progress has been made with gigantic strides in many directions, in engineering and in mechanics generally; while railways, steamboats, and electric telegraphs have extended their wonders to the most distant parts of the world; and while trade, with these aids, is bringing to our shores the produce even of the most distant places, to add to our comforts and our luxuries; yet, when we come to look to our homes, to the places where most of our population have to spend nearly the whole of their lives, I think we must find, with regret, that, in matters pertaining to the salubrity and general amenities of our towns and houses, as places for residence, due progress in improvement has not been made. Our house drainage arrangements are habitually disgracefully bad; and this I proclaim emphatically, alike in reference to the houses of the rich and the poor. We have got, since the early part of the present century, the benefit of the light of gas in our apartments; but we allow the pernicious products of combustion to gather in large quantities in the air we have to breathe; and in winter evenings we live with our heads in heated and vitiated air, while our feet are ventilated with a current of fresh, cold air, gliding along the floor towards the fireplace to be drawn uselessly up the chimney. A very few people have commenced to provide chimneys or flues to carry away the fumes of their more important gaslights, in like manner as we have chimneys for our ordinary fires. In mentioning this, however, as a suggestion of the course in which improvement ought to advance, I feel bound to offer a few words of caution against the introduction of flue pipes for the gas flames rashly, in such ways as to bring danger of their setting fire to the house. People have a strong tendency to require that such things as these should be concealed from view. In this case, however, special care should be taken against rashly placing them among the woodwork between the ceiling of the apartment and the floor of the room above or otherwise placing them in unsafe proximity to combustible materials. In many cases it would be better to place the flue exposed to view underneath the ceiling, and by introducing some accompanying ornamentation, to let the flue be regarded as a beneficent object not unpleasing to the eye.

The atmosphere of our large towns, where people live by hundreds of thousands all the year round, is not yet guarded against needless pollution by smoke, jealously, as it ought to be. Many of the wealthier inhabitants take refuge in living in the country, or in the suburbs of the town, as far away as they can from the most densely built and most smoky districts; but the great masses of the people, including many of all ranks, must live near their work, and for them at least greater exertions are due than have yet been made towards maintaining and improving the salubrity and the amenities of our towns. As to the abatement or prevention of smoke from the furnaces of steam-engines, the main requisites have long been very well known; but sufficient energy and determination have not yet been manifested towards securing their due application in practice. In too many cases futile plans have been tried, and on being soon abandoned have left a strong impression against the trying of more experiments; and this may account in part for the introduction of real improvements having been so slow. Smoke occurs when fresh coal is thrown suddenly, in too large quantity at once, upon a hot fire. By extreme care a fireman may throw coal into his furnace so gradually as to make very little smoke; but mechanical arrangements for introducing constantly and uniformly the new supply of fresh coal have been devised, and several of these have been such as to reduce the smoke emitted to almost nothing. I have seen in the neighbourhood of Glasgow, at a large manufacturing establishment at Thorniebank, one method which is applied to about thirty ordinary 40-horse-power boilers, in which upwards of 100 tons of coal are daily burned, and from the chimneys of which not more smoke is emitted than from many a kitchen fire. This method is under the patent of Messrs. Vicars, of Liverpool, and it seems to work very well. It has been about two years in work there. It was introduced at a time when coal was exceedingly high in pric , as

much to effect economy in fuel as to prevent smoke; and although the first cost was somewhere about 130*l*. per boiler, the proprietor considers himself to be already more than recouped for his outlay, as a saving of fully 12 per cent. in the fuel consumed was effected. At the same works I have also seen in operation the method of Messrs. Haworth and Horsfall, of Todmorden, which has, I am told, in certain circumstances, some advantages over the other. In this, as in the other, the coal is fed in uniformly by mechanical arrangements. The mechanism is different in the two cases, but the result in the motion communicated to the coal is very much alike in both. The bed of coal, which is gradually supplied in front, is caused to travel along the bars towards the inner end of the furnace, and the combustion proceeds in a very uniform manner in conditions highly favourable to economy of fuel, and without the emission of almost any visible smoke.

These two methods I have mentioned because they appear both to work very successfully in practice, while they both bring into effect the principle of action of the fuel which has long appeared to me to be the best that can be adopted for ordinary cases of steam-engine boilers.

I have now occupied, I think, enough of your time, and so I will conclude. I have endeavoured to select out of the wide range of subjects which fall within the scope of the Mechanical Section of the British Association, a few which have come more particularly under my own notice, and on which I thought it was in my power to give intelligence that might be interesting as to past progress, and suggestions that might be useful towards extension of improvements in the future.

SCIENTIFIC SERIALS

Archives des Sciences Physiques et Naturelles, No. 198.—M. C. Marignac contributes a paper On the simultaneous diffusion of certain salts, and gives long tables of the results of his experiments.—M. Marc Micheli gives a note of eighteen pages in length, On the Onagraceæ of Brazil, of which the greater part is taken up with the genus Jussiæa. He sums up the distribution thus:—

		N. America.	Mexico.	Antilles.	Guyane.	Pacific States.	Brazil.
Eujussieæa	23	1	2	5	7	7	22
Oligospermum	12	2	3	4	5	4	10
Macrocarpon	4	1	2	2	2	2	4
		4	7	11	14	13	36

—M. Maurice de Tribolet gives a concise history of the study of the genus Nerinæa, and gives analytical tables showing the distribution of species in the Jurassic beds of the Jura. The meteorological observations made at Geneva, under Prof. Plantamour, during May, conclude the number.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, Aug. 31.—M. Faye in the chair.—The following papers were read:—Astronomy at the Italian Spectroscopic Society, by M. Faye. This was a reply to some criticisms of P. Secchi. The author pointed out that P. Secchi's theory of sunspots was a return to the idea announced by Galileo in 1612, the clouds being buried in the body of the sun instead of floating above it. The theory advanced by the author on the other hand had been pronounced by Mr. Langley to be a *vera causa*. This *vera causa*, according to M. Faye, is nothing more than a law of hydrodynamics, perfectly established for terrestrial air and water currents.—Remarks on the fish of the Algerian Sahara, by M. P. Gervais. The remarks refer to species of *Coptodon* and *Cyprinodon*, the former of which had been cited by M. Cosson as proving the continuity of the sheet of water which extended over this region.—Note on the development of the contractile coat of the vessels, an anatomical paper by M. C. Rouget. New researches undertaken by the author on amphibian larvæ establish beyond doubt the contractibility of the ramified protoplasmic cells observed last year in the vessels of the hyaloid membrane of the adult frog.—On winged *Phylloxera* and its progeniture, by M. Balbiani. The author points out the complete analogy between *Phylloxera vastatrix* and the *Phylloxera* of the oak.—New observations on the migrations of *Phylloxera* to the surface of the soil and on the effects of the method of submersion, a letter from M. G. Bazille to M. Dumas. The letter contained a note, published in the *Messager du Midi*.—M. P. Mouillefert addressed also a letter containing observations on the employment of the chief insecticides from experiments tried in the laboratory at Cognac and on the vines of the neighbourhood.—M. P. Rohart addressed a letter on the action exercised by the soil on insecticide gases.—Other communications relating to *Phylloxera* were received from MM. Delfan, A. Richard, Gauthier, L. Rousseau, &c.—On a physiological phenomenon produced by excess of imagination, a letter from M. P. Volpicelli to M. Chevreul. Two experiments were made with magnets upon nervous subjects, to see if the effects produced were really magnetic or due to the imagination. In the first experiment a piece of unmagnetised iron was shown to the patient, who immediately fell into convulsions. In the next experiment a magnet was placed in the hand of a nervous subject, who at the end of a few seconds became so over-excited that the magnet was removed. That the effect thus produced was due to the sight of the magnet was proved by hiding several powerful magnets in the chair occupied by the same individual, who when thus unconscious of their presence experienced no ill effect. M. Chevreul made some remarks *à propos* of the foregoing paper on certain other illusions, such as the divining pendulum and divining ring.—Remarks on recent researches concerning the explosion of powder, by MM. Roux and Sarrau. The authors pointed out the agreement between certain of the results obtained by them and by MM. Noble and Abel in their recent communications to the Academy.—New note on the tail of Coggia's Comet, by M. A. Barthélemy. The theory of a repulsive force emanating from the sun requires, according to the author, that the axis of the tail should always be a prolongation of the radius vector. With Coggia's Comet, however, as observed by M. Heiss on July 5, the tail made an angle of 160° with the radius vector. The facts appear to the author to be simply explicable by the hypothesis of an interplanetary medium submitted to the attractive action of the sun, through which medium the comet travels with an increasing velocity; fans and jets are supposed to be the result of the sun's attraction on the denser portions of the cometary matter.—On a new theory of the formation of comets and their tails, by M. Virlet d'Aoust. In 1835 the author suggested the hypothesis that comets were nascent stars—the internal and still incandescent portions shining through cracks in the dark surface. This view was afterwards abandoned for Saigey's hypothesis, which considered the tails of comets as the result of the reflection of their light on an atmosphere which they drew after them. This opinion was again modified to meet the researches of Weiss, Schiaparelli, Klinkerfues, and Oppolzer, who showed the connection between the comets of 1862 and 1866, of Biela and Pogson, and the annular meteor streams which give us the August and November shooting stars. The author then asked whether comets did not equally belong to rings which had given rise to their existence, and if the light emitted by their tails did not simply result from the reflection of light from the nucleus on to the cosmical particles which constituted the rings on which they seemed to depend. The recent researches upon Coggia's Comet confirm this view in the author's opinion.—On a new model of prism for direct vision spectroscopes, by M. J. G. Hofmann.—On some points in the anatomy of the common mussel (*Mytilus edulis*), by M. Ad. Sabatier.

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